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Havemeyer Hall :		
Construction.....	\$516,488	62
Equipment.....	53,474	86
	<u>\$569,963</u>	48
Engineering Building :		
Construction.....	284,075	50
Equipment.....	20,325	47
	<u>304,400</u>	97
University Building :		
Construction.....	842,887	85
Equipment of power- house and connec- tions.....	115,578	52
Equipment of gymna- sium.....	39,399	24
	<u>997,865</u>	61
Vaults :		
East.....	30,382	79
West.....	37,316	40
	<u>67,699</u>	19
Old buildings—Repairs and equipment:		
West Building.....	10,252	67
College Hall.....	5,113	34
	<u>15,366</u>	01
Insurance.....	3,754	40
Outside street work.....	133,367	81
Improvement of grounds and inci- dentials.....	403,373	75
Expenses of removal.....	59,987	56
Interest.....	339,812	08
Total .....	<u>\$6,879,011</u>	90

AT Columbia University Mr. J. H. McGregor has been appointed assistant in zoology; Mr. S. O. Miller, assistant in mechanical engineering, and Messrs. F. S. Hyde, E. J. Riederer and Victor Linher assistants in analytical chemistry.

MISS ROXANA H. VIVIAN has been given the Alumnae fellowship for women in mathematics at the University of Pennsylvania, and Mr. H. B. Alexander, a Harrison fellowship in philosophy.

THE Hon. John McGregor has given £500 to the fund for the endowment of a chair of forestry in the University of Edinburgh.

DR. FERDINAND FISCHER, of Göttingen, has been promoted to an associate professorship of chemistry. Dr. O. Seeliger, of Berlin, has been appointed professor of zoology in Rostock and Dr. Haussner professor of mathematics at Giessen. Dr. Lorenz and Dr. Keller, of the Polytechnic Institute at Zurich, have been appointed to full professorships of electro-chemistry and zoology, respectively.

#### DISCUSSION AND CORRESPONDENCE.

##### THE METHOD OF TYPES.

IN discussing the outlook for stability in generic nomenclature\* the method of types has been contrasted with what might be called the method of concepts. The notion of the individual naturalist regarding a certain genus at any particular time is, of course, conceptual, but with increasing knowledge this concept is subject to frequent change resulting commonly in nomenclatorial confusion. With the older naturalists each concept was given a distinct name, while modern practice is less logical in employing a single designation for numerous and varying ideas, to the great detriment of systematic study, since the definite location of genera is rendered theoretically impossible.

The method of concepts originated in the days of mediæval scholasticism, when abstractions commanded great respect and were the subjects of careful study. It was based on the doctrine of the separate creation of species and logically comported with that view of nature. A genus could not be more than an abstraction under a belief which held, in last analysis, that the most similar species were equally distinct with the most diverse. Not only were genera thus assailable, but destructive criticism threatened even the specific idea, as witness certain applications of the oft-quoted assertion of Goethe: 'Nature knows only the individual.' While such ideas obtained, classification could have no logical sanction, its only utility being that of an index giving access to names and descriptions. A system which could do this with the least effort and the greatest dispatch became popular because its users enjoyed a sense of rapidly-expanding knowledge, and much convenience was afforded collectors who preferred their specimens with names. In the manufacture of classifications for this purpose adroit statement often proved more successful than careful study or deep insight. The problem was, indeed, entirely conceptual, the most diverse facts becoming of identical significance if they could be covered by the same formal statement. The resulting conventionalized ideas still figure largely as 'characters,' so that many

\* SCIENCE, August 12, 1898, pp.

existing systems of classifications are simply card houses of abstractions.

Whatever doubt may yet remain as to the causes and machinery of evolution, the doctrine of the separate creation of species is generally relegated to the past. The causal and genetic relationships previously supposed to affect only individual lines are now ascribed also to genera, families and orders. Systematic biology may not remain an index of names; it becomes an integration of all our knowledge of organic nature. There was introduced an element of finality which broadened all lines of study and suggested new interpretations for every fact and phenomenon, and yet the universal advance which this change of base rendered possible has not caused systematists to reconstruct their science in harmony with prevailing views on the origin, differentiation and distribution of life. In other words, there has been no general or consistent attempt at the application to classification and taxonomy of the pertinent corollaries of evolution.

Instead of an infinite array of disconnected forms and facts, only to be combined in artificial concepts, we may proceed in the study of any group of organisms with the assurance that a truly natural or phylogenetic classification is possible, and we have the further satisfaction that such an arrangement when reached will be final and command universal acceptance. The task of the systematist is no longer to study and contrive artful arrangements of concepts, but to gain the clearest possible insight regarding the form, structure and activities of the subjects of his study. All similarities and all differences have value and bearing, not merely those which have been previously used in classification. The deductive method must here, as elsewhere, give place to the inductive. Classification must be built up from below on facts, not suspended from above on abstractions. Affinities must be demonstrated by the production of the connecting links, not inferred from agreement in formal characters. No matter how obscure it may now appear, every species has its history and its relationships, which the naturalist undertakes to discover and to express by his systematic arrangement. He must be ready to accept, record and utilize every new

fact and recognize its bearing on the interpretation of other facts. From one group of organisms an extensive series of concepts may be drawn, and these will be successively advanced and thrown aside, the variable element being our knowledge, while the organisms furnish the constants which our notions should gradually approximate. The excellence of the systematist will depend upon his facility in the construction of new concepts in accordance with constantly changing bases of judgment.

Here is the practical issue regarding the method of types. Shall we continue the practice of naming the concept? Shall we not rather think of the name as applied in the most immediate and permanent manner to the organism? We shall thus have a designation ready for the final entity, but also available for any number of approximating concepts which may follow each other with no unnecessary confusion. Successive systems of classification may then be introduced with a minimum of bibliographic labor on the part of the specialist and a minimum of misapprehension for subsequent students. To maintain the use of the method of concepts because systematic biology had a mediæval origin is to stop the dial of progress and decree permanent confusion. And yet this is the purport of the prevalent systems of nomenclature. No existing legislation requires that a genus be anchored to any one point or vicinity. It is not merely a concept, but a negative concept, since it stands at the mercy of all comers, who may dismember at will and remove any of the species without apology. Certain codes will not permit the sequestration of all the original species, but systematists are not always thus considerate and some do not hesitate to take even the last, though they may avoid injury to the concept and use it later for a second set of species. Such jugglery has done its part toward bringing systematic biology into its present disrepute, for while all workers have not followed counsels of confusion, all are at the mercy of the bigoted and the reckless, and while there are many laws determining trifles of spelling and punctuation, there are as yet none dealing adequately with the weightier matters of clearness and permanence in the application of generic names, nor are there

rational provisions for the incorporation of new facts into systems of classification. At present each of these lays its claim to a finality which all past experience declares elusive.

Under the method of types we shall also be emancipated from the notion that all the species of a genus must of necessity possess a certain character or set of characters, and rational methods of description may be introduced. Natural groups are to be recognized and pointed out not only by means of an absolute agreement among their components, but also from the coherence of the parts. A natural genus may frequently be best distinguished from its neighbors on different sides by different characters, and the attention of the careful naturalist is directed toward the discovery and indication of the separating chasms as of the greatest importance in generic demarcation. Under the method of concepts the existence of genera without absolute definitions cannot be consistently admitted, but the method of types renders their stable designation entirely practicable.

Because it is philosophically true that we know things only from their characteristics, it does not follow that the set of formal characters by which we attempt to describe a generic group need be looked upon as the genus, since genera do not exist because their species agree in the few characters touched upon in the description, but because they agree in all except the superficial or quantitative specific differences. Thus, until a group of organisms is thoroughly known, the work expended on elaborate generic definition is largely wasted, each accession of new forms requiring an extensive readjustment. Under the method of types the energy spent on these temporary contrivances could be applied in the interest of a wider knowledge of the group, and that on a permanent basis. A revision of the genera of an order or family would consist in the thorough description and graphic reproduction of typical species rather than in the rehashing of concepts. Differences between naturalists cannot be expected to cease until knowledge becomes complete, but with the attention directed to the type rather than to the concept there would be the important advantage of a fixed and definite

point where all is now uncertain and fleeting. The question of recognition of two related genera would depend on whether the two supposed series or groups of species really stand apart, that is, whether there is a break between them. Nomenclaturally the difficulty is reduced to a minimum. If I recognize one genus the name is A—described in 1820; if two the second is B—described at a later date. If half the effort spent in arranging discarded concepts had been applied to the better description of types our knowledge of genera would be far in advance of the present. The amount of futile labor in systematic biology is appalling, and its practical results are to hamper the naturalist and to close the door of many departments of the science against all who have not a large amount of time for preliminary study of books with little relation to present knowledge or views of classification. Changes of names render the transition from popular to technical literature very difficult, and some considerate systematists have on this account opposed further alterations, even when logically required. Under the method of types it would be possible to select for the illustration of general and popular works species whose systematic standing is of assured permanence.

These are some of the practical benefits which would flow from the application of the method of types, in addition to a nomenclatorial stability out of the question under the method of concepts. Many active systematists are already using various modifications of the suggested method, but its consistent and thorough application under any uniform rule which would eliminate the variable factor of individual judgment, preference or prejudice is apparently barred by the fact that extensive initial changes in nomenclature would be necessary. That many carelessly applied names have escaped into popular use is not, however, a good reason why systematic biology should be kept permanently in its present confusion. The vast majority of names are known, as yet, only to systematists, and the next generation can learn new ones as readily as old, while present workers can well afford an occasional changed designation in consideration of the practical advantages of the method of types.

However, if the specialists in any group can permanently agree among themselves in the designation of the types of genera now recognized, there is no reason why any changes need be made. No new difficulties are, indeed, introduced by these suggestions; but to carry them out would simply bring to definite expression the disagreement and confusion latent among systematists, and make plainer the fact that uniformity and stability stand in inverse ratio to the personal equation; which means that some uniform, and hence arbitrary, method of assigning types for fixing the application of the older generic names will probably be necessary, such as the use of the first designated species.

O. F. COOK.

U. S. NATIONAL MUSEUM,  
September 22, 1898.

#### THE SUPPOSED BIPOLARITY OF POLAR FAUNAS.

DR. JOHN MURRAY, in a recent paper,\* again mentions the supposed general likeness of a large number of organisms captured in the Antarctic seas to those found in the Arctic seas (p. 133). I should like to say a few words on this topic, since I have paid particular attention to this question, and have repeatedly endeavored to show that in most cases this supposed 'bipolarity' does not exist at all, while in others it is no true bipolarity, cases of true bipolarity being extremely rare.

Especially for the Decapod Crustaceans I have found that "not a single bipolar species is known."† This sentence is quoted by Dr. Murray (l. c.), and he tries to show its incorrectness by mentioning the close resemblance of *Lithodes murrayi* Hend. of the Kerguelen region to *Lithodes maja* (L.) from the North-Atlantic, and by adding that—according to a communication by Mr. Henderson—there is no better illustration of bipolarity than that furnished by the *Lithodidae*.

I cannot admit these objections, since they are not supported by the facts. We possess a very valuable monograph of the *Lithodidae* pub-

\*On the annual range of temperature in the surface waters of the Ocean and its relation to other Oceanographical phenomena. *The Geographical Journal*, August, 1898, v. 12, No. 2.

†Zool. Jahrb. Syst., v. 9, 1896, p. 585.

lished by Mr. E. L. Bouvier in 1896,\* and Mr. Bouvier has shown plainly—as I have maintained previously—that the chief distribution of the *Lithodidae* is what I have called *meridional distribution*; that is to say, a distribution in the direction North-South, along the western coasts of the continents. It is a true case of false or mistaken bipolarity, a connection of the Arctic and Antarctic range of this family being present along the western coast of America (and perhaps of Africa). Moreover, according to the key of species and the notes given by Mr. Bouvier (l. c. p. 24), *Lithodes maja* is not at all the most closely allied form to *L. murrayi*; but there are two other species which may claim this distinction, namely: *L. tropicalis* A. M. E. and *L. ferox* A. M. E., both from tropical latitudes off the western coast of Africa, where they have been found in depths ranging from 800 to 1100 meters. This fact again suggests a connection from the Arctic to the Antarctic seas along the western coast of Africa, and we see that true bipolarity in the family *Lithodidae* as well as in the genus *Lithodes* is wholly out of the question.

I cannot understand at all why Dr. Murray again and again calls attention to the supposed bipolarity of the polar faunas as a *striking feature* in zoogeography. Most of the cases introduced formerly as instances illustrating this bipolarity could not be maintained after a critical examination of the respective zoogeographical facts. Thus, among the Decapods this theory finds no support, as I have shown, and likewise the supposed bipolarity of the Holothurians (Théel) does not exist, since Professor H. Ludwig† states that "not a single species of the Antarctic fauna is represented in the Arctic fauna," and that "there is not even a resemblance of both faunas, but a great dissimilarity."

Thus we see that a critical examination lessens the number of the superficially recorded cases of bipolarity, and that my doubts as to the correctness of the bipolarity as a *prime law* or as a *striking feature* of distribution are fully supported, and I am convinced that a careful

\*Ann. Sci. Nat. Zool. (8) v. 1.

†Hamburger Magalhaensische Sammelreise. Holothuriden, 1898, p. 90, f.